RAC RESPONSES TO PEER REVIEWER COMMENTS

Task 3: Inputs and Assumptions

Radionuclide Soil Action Level Oversight Panel

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"Setting the standard in environmental health"

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Responses to Reviewers' Comments On RSAL Task 3 Report

Upon reading the reviewer comments for the Task 3 report, *RAC* noted instances where we may not have clearly described the boundaries of the soil action level project, and, in particular, the limitations of this Task 3 report. It seems appropriate to remind the panel and ourselves of some of these constraints prior to addressing the reviewer comments. Additionally, much of the discourse that occurs at panel meetings guides the decisions made by *RAC* with regard to parameters, scenarios, and general project course. These conversations are generally unknown to the reviewers, and though they facilitate *RAC* and the panel's understanding of where this project is headed, they can be difficult to capture in a technical report, other than to state that discussions occurred and decisions were made based on these discussions. *RAC* has tried to summarize much of this information in the reports as possible, but it is sometimes difficult to convey the full explanation to the reviewers. When a situation like this is evident in the reviewer comments, we will point out the source of the misunderstanding and do our best to make the report as clear as possible with regard to the decisions made.

It is important to remember that the soil action level project is severely limited by budget and time constraints. In light of these constraints, we have endeavored to do the best science possible, and realize that, at some point beyond the scope of this project, further enhancements to this work may be desirable.

The goal of Task 3 was to identify parameters in RESRAD, based on that model's selection in Task 2, whose values, when changed, impacted the outcome of the soil action level calculation in a significant way. We were forced to streamline our efforts in this area, and not use resources to determine either uncertainty or alternate values for parameters that were not sensitive to change. Only obvious parameters from this category that justified change were adapted. Naturally, the parameters that emerged as obvious were the ones closest to *RAC*'s previous experience with resuspension and surface soil properties. Given more time and resources, there were a number of parameters that might have been subject to some degree of change and/or development of uncertainty based on a more thorough literature review and some necessary professional judgement.

In the context of this project and for the benefit of the panel, we have used published numerical data for quantification of uncertainty, whenever possible. As a result, we have tried to restrict widespread application of professional judgement in the area of quantifying uncertainty. This approach has proven to be confusing in other areas of the project.

This reminder of the goals and limitations of Task 3 and the project as a whole provides a background for responding to the comments of the reviewers.

PEER REVIEWERS

Reviewer A

General Comments

The general effort to incorporate as much site-specific information as possible into the RESRAD code is appropriate and to be applauded.

A number of the parameter assumptions adopted in the report are questionable to this reviewer. Some that are questionable are discussed under specific comments. It is not evident that the parameter assumptions are based on the most thorough and critical review of the existing literature.

It is recommended that some experts (for example, Greg Choppin, Florida State on K_d assumptions) be consulted on the reasonableness of some of the parameter values and their uncertainties.

This review was perhaps less than adequate because travel commitments of the reviewer precluded a full, comprehensive review with detailed recommendations for additional sources of information.

Specific Comments

The K_d of 218 cm^3 g^{-1} for Pu seems at least two orders of magnitude too low. This value would not be consistent with the characteristics of Rocky Flats Soil, which is high in clay, nor with the observed behavior of Pu in the Rocky Flats environment. Furthermore, the GSD of 1.16 is way to low. This implies that the uncertainty on the value is quite small, which it is not. Secondly, ground and perhaps surface water are the main things this parameter would affect, so I am puzzled as to why this parameter was sensitive. However, a K_d of only ~200 would allow fairly rapid surface depletion of Pu, which would reduce resuspension. This could explain the sensitivity, although this was not explained, unless I missed it. However, this is even more confusing, since I think the approach is to use measured mass loading in any case to derive the inhalation exposures.

The K_d value for Am is also too small, I believe, but the GSD value seems reasonable.

I'm not happy with the way these values were derived in any case. Apparently, they trace back to retardation factors developed by Dames & Moore. I think much more can and should be done to come up with and justify more reasonable K_d values.

We are reviewing Actinide Migration Panel studies to further enhance our K_d evaluation.

The soil to plant transfer factors were listed as "sensitive" parameters. First of all, I am a bit surprised by this, since one would expect food chain exposure to be a very small fraction of the inhalation exposure. This needs some explanation. Secondly, it is not clear whether these values represent strictly root uptake, or a combination of root uptake as well as dust loading. If they represent strictly root uptake, I think there are data to indicate about an order of magnitude smaller value for Pu at least. If the values represent root uptake plus dust loading, then the values

are too small, by roughly an order of magnitude. I'm not certain how the computations are handled in RESRAD, but this needs to be explained.

Upon reviewing data for this report and the project in general, the recommendations of NCRP Report No. 129 were explored. It was decided early in the project to include the distributions for soil-to-plant transfer factors because of the inclusion of the agricultural pathway for some of the scenarios. These parameters are, however, only moderately sensitive to change, as pointed out by the reviewer. They were mistakenly included in the "most sensitive parameters" section because we planned to include a distribution. We will move the discussion on these parameters to the section titled "Parameters with Limited Sensitivity".

In RESRAD, the soil-to-plant transfer factors represent only that fraction of contamination that is transferred to plants via root uptake; the dust loading calculations are handled through the use of a mass loading for foliar deposition parameter and calculation.

The area of contamination is listed as 40,000 m^2. I think this is too small, but apparently, the computations are going to somehow use actual soil data in a spatial sense. It is not clear to me how this will be done, and whether or not the assumption of a particular area is even important, if this is to be handled in some spatial scheme that is not normally tackled by RESRAD.

The 40,000 m^2 area listed was the area used in the previous DOE calculations. The current calculations will derive an area based upon scenario assumptions and use this area and the contamination associated with it to develop air concentrations as indicated by the available monitoring data. This evaluation will be appended by a modifying factor, which will attempt to account for a situation in which groundcover is eliminated, making contaminated soil much more available for resuspension.

The mass loading estimate of 2.6 x 10^{-5} is reasonable for most rural locations. However, why is this even important to debate here if actual soil loadings are to be used? If actual soil loadings are to be used, what soil concentrations for the radionuclides are to be used, given that the source of dust would most likely be quite general?

The mass loading factor shown in the text is again the factor used in the previous calculations. Current calculations will utilize available information to develop actual soil loadings. The radionuclide concentrations in the soil currently, described in the section titled "Initial Concentrations of Radionuclides," will be used for the contaminated soil. Additional soil contributing to the soil loading profile will be assumed to result from uncontaminated soils in the upwind fetch.

The statement on page vii "High wind also results in lower air concentration than would be expected if the same material was dispersed over a longer period of time during average wind speed conditions." needs some explanation and documentation. This could be true, unless average wind speeds were insufficient to cause any measurable resuspension, due to good vegetation cover.

This statement comes from results of the dose reconstruction study done at Rocky Flats. This study predicted that although high winds likely resulted in a large degree of soil movement, the dispersion of this material was so great that the concentration of contamination in air was significantly less than that which resulted from average wind speed conditions. This dispersion effect is magnified close to the source, which is the location of the receptor in this study. The statement in the executive summary is expanded in the section of the report dealing with average wind speeds.

The value suggested for the depth of soil available for resuspension, namely 3 cm, seems way too high to me. Most studies have indicated that on a time scale of < 1 year or so, only a couple of mm are likely to be available for resuspension, unless the site is highly erodable due to overgrazing, lack of vegetation or mechanical disturbance.

The depth of soil available for resuspension represents the layer of soil within which contamination in uniform. The selection of this value was dictated primarily by the available soil data, most of which represented area to that depth. Although it would be desirable to represent the contamination in a shallower layer, the data available to us make it difficult to estimate contamination to any other depth. The research of Webb et al. (1997) showed that throughout the top 3 cm, the contamination is primarily uniform, with perhaps a slight dip in the contamination at the lower depths. Webb et al. also provide a means to convert contamination profiles at other depths to the 3 cm depth. Since we are constrained to this depth by the available data, we must use it for the depth of soil available for contamination. As erosion progresses, uniformly contaminated soil from the lower part of this 3 cm will likely be exposed to resuspension. We will incorporate a better description of this parameter in the final version of the report.

The assumption that the irrigation contamination fraction is 1.0, seems unreasonable. This needs more justification, especially since groundwater does not seem contaminated. What about surface water on the other hand? Is this included in the model?

As a part of an agreement reached with the panel overseeing this study, we agreed to include contaminated groundwater as a possible pathway for exposure. Since one of our exposure scenarios is a residential rancher, allowing irrigation water to be contaminated was an important possible pathway. Because we assume that the source of the irrigation water is directly from a groundwater well located beneath the site, the contamination fraction is set at a value of 1.0 to make the irrigation water as contaminated as the groundwater. The groundwater pathway analysis is included only as a screening calculation, to show the possible effects of groundwater at the site and to direct future studies.

Reviewer B

Review Summary

The content of the above named report is focused on a discussion of *RAC*'s chosen values for model parameters, the assumptions used to justify the choice of those parameter values, and on a sensitivity analysis of the soil action level calculation.

This report was organized in a reasonable way and sufficient detail was presented for most parameters. The Executive Summary seems rather long for a report of this length. Many of the chosen values for parameters seem reasonable, others in my view are not credible; each are discussed in the remainder of this review.

Being that the purpose of the report was to present the results of a sensitivity analysis (stated on p. v and p. 1), the report was not completely successful because the method of conducting the sensitivity analysis did not allow for the analysis to reflect the range of sampled values from each distribution (see my comment #8 below).

Other detailed comments are found below.

Detailed Comments

p. vi. The first of several times, it is stated that "soil-to-plant transfer factors quantify that portion of contamination in soil that is transferred to plants via root uptake". This is not a correct interpretation of soil-to-plant transfer factors. These factors represent the fraction of the concentration of the soil within the root-zone of the plant that is observed in plants – also on a concentration basis. Because of the much smaller mass of the plant relative to soil, it is *not* the fractional transfer of the soil inventory. Such incorrect statements appear on p. 19 and possibly elsewhere in the report.

Although the authors certainly had the correct definition in mind when writing the report, we thank this reviewer for noting this inconsistency with the appropriate definition. In an attempt to be as clear as possible for the majority of the audience of this report, we left open a door for misinterpretation of our definition. We will clarify this definition in the final report.

p. vii. It is noted that that RAC will use a 5-year average wind speed, etc. for modeling resuspension, but a few sentences later comments that a "distribution of wind speed values" will be used. It would be useful to explain here very briefly if the distribution discussed is a model of the uncertainty in the average or if not, to clarify the distribution.

We intend only to use the 5-year average STability ARray (STAR) met data for modeling the resuspension. Although we examined the change in the average from year to year, we discovered very little fluctuation in annual average. We intend to remove the statement from the executive summary that indicates the use of a distribution.

In the Executive Summary and elsewhere in the report (for example, see beginning sentence of Executive Summary-Scenarios), it states "The Task 3 report describes....". At this point, I had to look back at the cover to reaffirm that I was reading the Task 3 report. It would be better to

state, "This report describes...", thus, eliminating any confusion about which report is being referenced. This occurs elsewhere in the report.

We include statements like this for clarity, since we refer throughout the document to a variety of reports. We appreciate this comment, and will make the discussion as clear as possible in the final report.

On the top of p. ix, the authors state "RAC created distributions..." I suggest that the preferred technical language would be "RAC defined distributions...". This language appears on p. 18 and possibly elsewhere in the report.

We appreciate this comment and will incorporate this language.

p.2, last paragraph. Rephrase: "It is obvious that this single change in the RESRAD code has a large impact on the dose delivered by the resuspension pathway" *to* "It is obvious that this single change in the RESRAD code predicts a significantly different dose via the resuspension pathway".

We appreciate this comment and will incorporate this language.

I note from Table 1 that RESRAD Version 5.82 predicts a soil action level about 6-fold greater than does version 5.61. Such a dramatic change between what seem to be similar versions of the code (based on their version numbers) raises questions about the scientific basis for the resuspension calculation as well as other pathways in the code. It is impossible for external reviewers such as I to judge the validity of the code before or after such changes. This point is raised here as a precautionary flag to RAC and RSALOP that the technical basis for calculations in the code needs to be continually scrutinized as each version change is made.

We recognize this dramatic change as well. Although a perusal of the RESRAD documentation seems to indicate that the change in the code is warranted scientifically, we decided to utilize site-specific data in our evaluation of resuspension and create an external resuspension model rather than to use the one internal to RESRAD V. 5.82.

P. 4 notes that "a single parameter uncertainty analysis *requires* [my emphasis] that only one parameter be changed at a time." This is an overstatement in my view and sounds as if the ends justified the means. It would be more accurate to state that "a single parameter uncertainty analysis *is defined by changing* only one parameter be changed at a time." Moreover, single parameter uncertainty analyses are not regarded as state-of-the-art; I think that fact should also be given some note in the report. State-of-the-art sensitivity analyses vary all parameters simultaneously and rank the sensitivity of each parameter based on the fraction of the output variance contributed by each parameter. Such techniques are generally more difficult to implement. Techniques of lesser sophistication, such as that available in RESRAD, can be used, but their limitations should be noted.

The authors and the panel recognize that this sensitivity analysis is not state-of-the-art. A more rigorous analysis will be completed for Task 5 of this project, using the distributions defined in this report. However, the single parameter analysis was required

for this portion of the project, and, in fact, reveals the information about each parameter that we were looking for – how important is each parameter in the calculation of soil action levels and doses?

The metric by which sensitivity was judged was not mentioned in the report. Was it the absolute or relative change in the output?

Due to many comments regarding the sensitivity analysis, we will make efforts to more carefully describe it in the final version of the report.

Given that RAC has discussed the necessity of a dynamic (time-dependent) model for determining soil action levels, has the sensitivity to the set of parameters been determined over (future) time?

Part of the reason RAC prefers the modeling approach outlined here is because of the ability to evaluate soil action levels under a variety of conditions (e.g. current, remediated, future catastrophic event) that may be present at future times.

P. 4, The sensitivity analysis was not performed appropriately to determine the sensitivity of the model to the parameters and their specified distributions. The third paragraph states the "parameter values were allowed to vary by a factor of 10 in either direction." Sensitivity analysis is intended to show the sensitivity of the output variable to both the mathematical structure of the model and the legitimate range of variation of parameters. By presetting all parameters to the same degree of variation (10x in either direction), the sensitivity of the model to the variability of the parameter is lost. Only the sensitivity to the model structure is retained. Thus, from the results presented, it is not easy (or maybe not even possible) to see the true sensitivity of the model to each parameter. RAC should consider redoing the analysis.

***RAC* will not endeavor to redo the sensitivity analysis. We recognize that we are not evaluating true sensitivity to variability (or change) in the parameter, but rather to the model output's sensitivity to changes in the parameter value. This is, however, the sensitivity we were seeking to evaluate at this juncture of the project. Quantifiable variability in the parameters is designated in this report; sensitivity to this variability is a part of the final task of this project.**

Depth of Soil Mixing Layer (p. 5): RAC has selected the depth of 0.03 m (3 cm) as the depth of soil available for resuspension. This is certainly a better choice than the thickness of the contaminated zone (over which the concentration may vary substantially).

We agree and appreciate this reviewers comment.

Indoor dust filtration (p. 5): The definition of this is poorly stated in the same way that the soil-to-plant transfer was poorly stated. In the two opening sentences, "contamination" should be changed to "concentration" because "contamination" is too vague and could imply inventory, which is definitely not equal to concentration (since the volume of the house is much smaller than the volume of the atmosphere!).

Again, we appreciate the suggestion for clarification of our definition and will make the appropriate adjustments.

Moreover, RAC assigns an equal value to the indoor air concentration, notes it is a conservative assumption, assigns no uncertainty, and states *a priori* that they will not change the value. This is the first of several locations, where RAC fails to produce a credible uncertainty analysis due to the assumptions they make. The noteworthy problems in their method are as follows. 1) An uncertainty analysis should determine credible bounds around a realistic central value of the model output (in this case, the soil screening level). It is impossible to determine credible bounds on the output parameter when some input parameters are set to "conservative" values (in other words, higher than likely) as these parameters will skew the entire result toward larger and unrealistic values. 2) Assigning no uncertainty to a parameter is the same as stating confidence in the value. No one could possibly assert confidence in the assumption that indoor concentrations equal (exclusively and without variation) the outdoor concentration. 3) An uncertainty analysis *requires* (and *requires* is used correctly here) that the assessor be unbiased in choosing parameter values and be impartial to changing those values, as is dictated by the science. This is clearly not the case here as RAC as chosen to purposely maximize the pathway (that is the meaning of choosing *conservative* values) in the interest of not underestimating the inhalation dose.

***RAC* appreciates this reviewer's comments about uncertainty analyses, but we do feel it is important to point out a few key elements of this project that dictate the direction we must take. First of all, it is important to remember that the purpose of Task 3 was to evaluate the input assumptions assigned to RESRAD parameters as they were used in the prior analysis (DOE/EPA/CDPHE 1996). This boundary condition on the analysis was put in place because of two important factors: 1) The panel was interested in knowing how the values selected for the previous analysis affected the calculation, and 2) the limitations on this project prevent us from doing an analysis such as that suggested by the reviewer. In light of these two factors, the sensitivity analysis was set up in such a way as to maximize our resources and minimize our effort on parameters for which a credible value had been chosen for the previous analysis.**

It does not follow that assigning no uncertainty to a parameter is the same as stating confidence in the value. What it means is that, under the limitations of this project, we saw no reason to change the parameter from its previous value. In the case of the indoor air concentration, the value used in the previous analysis, 1.0, was determined to be reasonable given that we know very little about the future conditions at the site.

Based on the comments of a number of reviewers, however, we plan to examine a distribution of values for this parameter.

Irrigation Water Contamination Fraction (p. 5): The same comments as Indoor dust filtration apply here.

This factor was discussed in the set of review comments from Reviewer A.

External Gamma Shielding Factor (p.6): Equation 1 describes a *weighted shielding factor* and not an *occupancy factor* (which is the fractional time spent indoors). I don't know whether

RESRAD is responsible for such poor names for variables or if it is RAC's choice; either way, it is incorrect.

The variable name "occupancy factor" is one that was assigned by the RESRAD designers and is cited in the documentation for the code. We will continue to use it in our text.

What is the uncertainty assigned to the shielding factor of 0.7 chosen by RAC?

We assigned no uncertainty to the shielding factor, as it was a parameter that exhibited almost no sensitivity.

p.8, It is noteworthy that RAC has chosen to explain that the research results of Los Alamos National Laboratory indicate that plutonium in the soil is insoluble. The interpretation should be that plutonium will, thus, not enter the ground water. RAC gives less commitment to that interpretation and states that plutonium "may not get into the groundwater." It is difficult to provide advice here except to note that it should be possible to incorporate a multiplicative parameter(s) to represent both the likelihood of water contamination as well as the degree. Maybe this has been done but it is not clear to me if it has.

Since we have committed only to completing screening calculations for the groundwater pathway, with the recommendation that future research be directed toward refining this calculation, we will not incorporate a calculation of this type. We will complete a calculation for the resident rancher scenario that includes the groundwater pathway, as well as one that incorporates only inhalation, with the understanding that the groundwater calculations are not definitive, but rather indicative of potential for dose.

Table 4. Soil-to-plant transfer factors should be noted to be chosen from NCRP 129 *recommendations*, not *data*.

We will note this in the text.

Units of pCi/g are used for the initial concentration in Table 4. Units of Bq/g should be used, though I am sure that RESRAD is probably to blame. In either case, it is inexcusable. Later on in the report (e.g., in Fig. 4), SI units are used. A consistent set of units throughout is preferable with SI being the preferred system.

Throughout this project, it has been difficult to stick to SI units, because the panel commonly prefers more recognizable units. We will insert both SI units and the readily recognizable conversion in all tables and within the text of this report.

The same comments as discussed in point number (9) above, apply to the parameters of "Plant food, contamination fraction" and "Drinking water, contamination fraction", both which are assigned a value of 1.0 in Table 4.

The value for plant food, contamination fraction was completely insensitive to change, so we left this parameter at its previous value. The drinking water pathway has been included only in a single scenario for the purposes of a screening calculation, and is intended to be conservative.

Groundwater/Drinking Water Pathway. It appears from this discussion that the parameter named "contamination fraction" refers to the fraction of the drinking water consumed that is contaminated. This is extremely vague. Does that imply that all water consumed is contaminated and only has a single concentration (that is, it never varies)? The assumption of 100% contamination with no assigned uncertainty is not credible.

Drinking water with a contamination fraction of 1.0 will come strictly from a groundwater source. The concentration will vary with the concentration of the ground water. As described above, we intend for any calculations that include groundwater as a source of drinking water or irrigation water to be conservative, bounding level calculations only, as a means of evaluating the potential for dose.

Furthermore, the chosen value of 2 L/d of contaminated drinking water is not realistic, but overly conservative. In regulating drinking water contaminants, EPA uses the value of 2 L/d for adults and 1 L/d for infants (10 kg body mass or less) as default values only. However, the most commonly cited study on water intake is that of Ershow and Cantor (1989, Total Water and Tapwater Intake in the United States: Population-Based Estimates of Quantiles and Sources, A report prepared under National Cancer Institute Order #263-MD-810264. Bethesda, MD: Federation of American Societies for Experimental Biology, Life Sciences Research Office) which estimated daily water intake and tapwater intake by age and gender. They defined "tapwater" as "all water from the household tap consumed directly as a beverage or used to prepare foods and beverages" and "total water" as tapwater plus "water intrinsic to foods and beverages". Values as great as 2 L/d can only apply to total water intake.

The all age-averaged median value for tapwater intake by males is about 1.1 L/d, and about 1.05 L/d for females. RAC should determine if gender and age-dependence will be accounted for. Regardless if age and gender-dependence is accounted for, realistic values for the population median tapwater intakes are only about one-half or less of RAC's presently assumed values.

Based on the above comments, the doses estimated in paragraph 4 are unrealistically too large.

Again, the use of drinking water in the soil action level analysis is done only to evaluate potential for dose of this pathway. It would not be possible, given the constraints of this project, to evaluate dose or soil action level including this pathway in any definitive way. We make these calculations and draw attention to this pathway only as a means of highlighting all of that which we do not understand.

P. 15. Change "daughters" to "progeny."

We agree and will make this change.

UNCERTAINTY DISTRIBUTIONS. The opening discussion of this section does not represent a state-of-the-art description of uncertainty analysis and the sources of uncertainty, as no distinction between uncertainty and variability is made and "uncertainty" and "variability" are sometimes misappropriately interchanged. RAC should be aware of IAEA Safety Series Report 100 (about 1990) or Hoffman and Hammonds (1994), "Propagation of uncertainty in risk assessments: the need to distinguish between uncertainty due to lack of knowledge and uncertainty due to variability", *Risk Analysis* 14, 707-712. This section should be rewritten to better distinguish uncertainty and variability.

At this place in our work, we do not plan to rewrite this section. The authors of this report have defined uncertainty within the context of the tasks we are accomplishing. Because of the implications of the results we will provide, we do not endeavor to quantify that uncertainty that results from lack of knowledge, particularly in the context of the groundwater pathway. We simply will not provide a set of soil action levels resulting from exposure to this pathway when so much about the transport within the saturated zone is not known. Our representation of uncertainty as encompassing variability is appropriate for this project and will be maintained.

Distribution Coefficient (p. 17): I adamantly disagree with the authors reference to "unquantifiable uncertainty." This is a prime example of the confusion between uncertainty and variability. For example, it may indeed be difficult to determine the extent of *variability* of this parameter (though there are numerous measurements reported in the literature). The uncertainty, however, can be estimated by the assessor (RAC in this case) based on whatever evidence and expert opinion they have. There is no single correct estimate of uncertainty as implied here, in other words, uncertainty is always quantifiable based on available evidence and judgment.

The discussion on the bottom of p. 18, which disregards certain data of Krey and Hardy (1970), Krey et al. (1977), is troubling. It is not possible for this reviewer to determine the legitimacy of RAC's analysis here. It is worth noting that Krey and Hardy had many years of study Rocky Flats contamination and they represented the finest sampling and environmental lab in this country, while the analysis of Rood (1999) is presumably a literature review. I recommend that RSALOP contract Krey as a reviewer of this material as well as of the Rood (1999) report. Krey is retired but can be contacted through the U.S. Department of Energy Environmental Measurement Laboratory in New York where he formerly served as Director.

RAC appreciates the comments of this reviewer, but we continue to assert our position about the degree to which the uncertainty about transport into groundwater is quantifiable. We appreciate and recognize all of the available data on transport into groundwater, but, for the benefit of the panel and this project, believe it is premature to evaluate uncertainty in this pathway and present a set of results that can be interpreted as applicable to the determination of soil action levels from this pathway. We would be remiss not to refer to the available research on the topic, but will not, at this time, present results with uncertainty bounds that have the opportunity to be misinterpreted outside the context of this study.

In the section on page 18, a discussion of K_d values takes place. We have obtained new data and are reviewing it for inclusion in this section.

P. 22, It is unclear what is meant by "RAC made adjustments to bring samples from various depths into conformity with the profile of Webb et al." Though it sounds like intentional manipulation of the data, it is probably more benign than that, but still not clearly explained.

What RAC has done is use the available concentration profiles reported by individual researchers and determine what the concentration in the top 3 cm was based on these profiles. In some cases, concentrations over depths larger or smaller than 3 were reported. In these cases, the fractional concentration depth profile provided by Webb et al. (1997) was used to adjust samples taken at different depths to a common depth of 3 cm. This is described in the text on page 20.

P. 22. It is unclear what RAC means that much of the data of Litaor could not be documented. I personally knew Mr. Litaor and he is an extremely thorough and careful researcher. Possibly the statement means that necessary ancillary data or sources of information was not provided. Mr. Litaor, however, can be contacted at his new employer in Israel for further information and I suggest that be pursued. His more recent publications in Health Physics give his present address.

We also have been in contact with Mr. Litaor throughout the course of this project. It is, in fact, because of Mr. Litaor's help that we were even able to obtain the database of values that he provided. We had some trouble, without constant contact with Mr. Litaor, discerning the depth to which some of the soil sample data provided were collected, because the references to the data were not readily attainable. Even after discussions with him, it was clear that the data provided to us were not separated within the database as to sampling depth. One set of data in which we were particularly interested was collected over "various depths up to 5 cm." The only option available to us was to assume the same depth of sampling (5 cm) for the data that we were not able to document. In this section, we simply warn the reader of the limitations of our data set. For the purpose of our spatial model, which is to provide a basis for integration of resuspension over large areas, the data set was sufficient. We continue to try to resolve these difficulties.

P. 24, The opening sentences describing a spatial model seem to me a bit elementary and imprecise. It would be better to describe that a spatial model is primarily intended to explain and/or predict the observations, thus allowing for predictions to be made at locations without observations with a reasonable level of confidence. Whether or not the model provides smoothing is entirely optional. While most do, I certainly don't agree with the statement that it *must* do so.

The uniformed reader might be led to assume that the two methods (kriging as used by Litaor) and determination of power functions within polar sectors (as used by RAC) are equal. They are not, as their origins and technical basis are so different, it is difficult to compare. Kriging intentionally takes advantage of the spatial correlation of data and uses that to an advantage when predicting values at locations where no observations are available. RAC notes that in two sectors (292.5° and 315 °), there was too little data to determine the functions, thus RAC assumed the functions from a nearby location (270°). It is worth pointing out that kriging would base these locations on the spatial trends, rather than on an assumption. I am not suggesting that RAC revise their methods of spatial interpolation to kriging (which is a much

more difficult mathematical technique) but am pointing out that it could be of some advantage, such as in the situation noted here.

We intentionally selected the power function analysis to base our contour smoothing on the assumption that the spatial signal was the result of wind transport of contaminated soil particles from the 903 Area. A kriging analysis was not justified in the context of what we were trying to accomplish.

Fig. 4. Along a west-east line at coordinate of Northing 441.0, there is a line of measurements that are all gray circles (10-100 Bq kg^{-1}), yet they fall well outside the 2 Bq kg^{-1} contour. Where is the discussion explaining these measurements and the lack of agreement of the contours with the measurement data?

As with any model, the model described here is not capable of predicting every measurement. Because our model based the spread of contamination on the assumption that wind transport was responsible for the spread of contamination, there are measurements outside of our wind transport contours that likely resulted from other contamination events at Rocky Flats. Evidence from the dose reconstruction studies as Rocky Flats might give us some insight into the source of these above background readings. A fire at building 771 at the Rocky Flats plant in 1957 released a significant amount of airborne particulate contamination. Meteorological data from that period indicate that the wind direction probably directed the contaminant plume in a southerly direction before the wind direction shifted and the plume proceeded to the northeast. Although particle size of contaminants was very small and little deposition probably occurred in the aftermath of this event, it is likely that the measurements taken at these locations resulted from contamination from the 1957 fire.

p. 28, RAC states they "will estimate the variation of the air concentration that exists within the defined domain based on the current state of ground cover, using the existing air concentration data." I have two questions about this statement. 1) The air concentration data can obviously be used to estimate its own variation. Is there something more important being said here? 2) RAC has claimed in the past the importance of using a dynamic model (which implies incorporating a time-dependence to estimate values likely in the future). How will the current state of ground cover be extrapolated to the future for the purposes of dynamic modeling?

The current state of ground cover gives us an important stepping off point. To this estimate of dust loading determined using the available soil and air concentration data, we can apply an enhancement factor that uses the resuspension studies completed at Rocky Flats to estimate the increase in dust loading that might result from an event that would remove the available vegetation cover.

p.28. The paragraph beginning "A procedure such as this..." needs rewording. Obviously some words are left out which render the paragraph unintelligible.

It appears a word was left out of this paragraph during review. We thank this reviewer for bringing it to our attention.

p. 28-29. The discussion on the fetch of airborne dust incorporate opinions of RAC ("these distances seem to short to be consistent...") and the validity of those opinions versus the findings in the literature is a very technical matter. I suggest the RSALOP contact Dr. Joseph Shinn to evaluate this discussion. It is important and deserves an opinion of greater expertise than my own or anyone on the RAC team. Shinn can be contacted at Lawrence Livermore National Laboratory in Livermore, CA.

p. 29-30. The discussion of mean annual wind speed seem reasonable to me though the findings are outside of my expertise. The opinion of Dr. Shinn would also be valuable here.

While this project would certainly benefit from a rigorous review process including reviewers from a number of arenas, there is a time limitation that will prohibit additional review at this stage of the project.

P. 32. The full-time resident rancher is an unrealistic scenario, being that the assumption is that members of the family never leave the site. As a reviewer, possibly I have not been given an adequate briefing on how the scenarios are to be defined and used, but such assumptions are not realistic and contribute little to an understanding of the risks of RFETS. I recommend changing all unrealistic assumptions because they have no face validity and no place in the application of probabilistic risk assessment. Such scenarios do not require peer review because they have no basis on which a review can be conducted. I do not endorse these values or any unrealistic scenarios.

It is important to understand the context of the development of the scenarios, which were carefully established with the help and consensus of the panel. The process by which these scenarios were developed was long and involved. That process can not be fully outlined here, but suffice it to say that the scenarios have been carefully thought out by both RAC and the panel, and represent our collective view of reasonable scenarios for a future that is impossible to predict.

Table 10 is a summary of parameter values, most of which have been commented on above.

The number of days per year in which soil ingestion is assumed to take place is excessive. Northern Colorado where RFETS is located, normally experiences cold weather that would make it impossible for a child or infant to have access to soil every day. Protection of the public can be adequately ensured by setting the upper end of the distribution equal to 365 days, not the median.

Again, the panel has decided upon this value, which is a constant.

P.34. The first paragraph on this page explains the review of literature data, defining distributions, etc. The 2nd paragraph attempts to explain, but actually glosses over without adequate explanation, a very important concept. Here it is described how a percentile is selected and the rest of the data disregarded. It appears that a single value of each parameter is chosen which RAC believes is protective of the population and the entire set of single values (one for each parameter) are then used to calculate the soil action level (I assume). The question is: How reliable of an estimate is produced? It has long been known that choosing conservative values for

all parameters results in a highly exaggerated final result. Possibly I have missed something, but I don't understand this process and I express great concern over what is written here.

This technique was discussed and agreed upon by the panel.

I note further that the last sentence of the report (p.41, "Values for the soil action level and dose will be presented as distributions of possible values for each individual scenario") seems not to be in agreement with the process of fixing values as described on p.34.

We intend to fix values only for the scenarios (Table 10), allowing the parameter values that fall outside of the boundaries of the scenarios (Table 4) to vary. This will provide a distribution of doses and soil action levels for each fixed scenario.

Breathing rate, 2nd paragraph (p.34). The word activities is overused in this sentence ("...the activity levels for indoor and outdoor activities differed").

We will adjust our word use in this sentence.

Groundwater (p. 40). RAC has chosen to evaluate contaminated groundwater as a source of exposure and this seems like a reasonable thing to do. RAC should be cautioned, however, that their last statement ("Failure to address these pathways quantitatively leaves open the question of their potential importance") implies that they are interested in correctly *quantifying* the risk. For that reason, they should use all of the quantitative evidence, including the insoluble nature of plutonium as assessed by Los Alamos National Laboratory. Ignoring any evidence will defeat the process of correctly quantifying the risk.

We will change the wording in this sentence to reflect our intent to provide a screening level calculation, not a quantitative risk evaluation, for the groundwater pathway.

Drinking Water Intake (p.40). I have already addressed the overestimate of water intake that RAC proposes. Does Layton (1993) really address water intake? I only remember that it discusses inhalation rates.

Thanks to the reviewer for noting that the reference was not the appropriate one. The correct reference is *Finley et al. 1994*, and the change will be made in the final report.

Reviewer C

Introductory Note: for convenience, overall comments are presented first, and more detailed comments are presented on a page-by-page basis. Purely editorial comments are introduced by the word "Editorial". From my perspective, RAC need not respond in writing to any of the comments and suggestions labeled "Editorial".

Overall Comments

This is a well-conceived, well-presented and well-written draft, and was a pleasure to read. It is important that this task is in very good shape at this stage, since arguably it is one of the most important in the whole project. There were very few typographical errors, and only very few sections merit substantial re-writing or additional content for improved clarity and comprehension.

The Executive Summary was particularly excellent. Anyone who reads and fully understands the Executive Summary has a very good understanding of the entire report.

I recommend that a paragraph providing an overall perspective be added to the Most Sensitive Parameters section in the Executive Summary. It should provide RAC's general view on the reasons it has chosen different values for the five parameters, such as: RAC is using more recent or more extensive data, DOE/EPA/CDPHE did the best they could at the time, DOE/EPA/CDPHE really chose poorly for some of these five parameters, DOE/EPA/CDPHE badly botched the job back in 1996, etc. This perspective will be very important for the non-specialist reader who reads only the Executive Summary of the results of this task. If such a perspective is not provided, it will leave each reader free to draw his or her own conclusion from among the choices I listed. As an example, later on page vi, RAC clearly points out that for the soil-to-plant factor, RAC used a more recent definitive report, which was simply unavailable in 1996. This choice would be understandable to and accepted without question by all but the most cynical and suspicious readers, and should be part of the overall perspective that I recommend be added to the Executive Summary.

A very good suggestion for improvement of the executive summary, which we intend to take.

Detailed Comments

Page v, end of second paragraph. Either here or somewhere in the Executive Summary there should be a brief description of: a) the major conceptual difference(s) between RESRAD 5.82 and Version 5.61 used in 1996 and/or b) the major differences between the two versions as they relate to this specific project. See, for example, page 2, 2nd paragraph, where this is dealt with.

We will incorporate this enhancement

Page v, last paragraph. This paragraph, which introduces RAC's "bottom line" values as shown in Table ES-1, should be expanded to provide a little more explanation of how RAC

reached its values, or else it should alert the reader that the reasons for any differences will be explained in detail later.

As a part of the general comment above, this will add to the clarity of the executive summary, and we will incorporate a discussion like this.

Page vi, first paragraph, next to last sentence. There should be a brief description (a phrase would do) explaining why RAC's value for uranium is four times higher.

We continue to explore the topic of K_d values, using more recent data from the actinide migration panel. We will present final values in the final report.

Editorial, page vi. Is there a need for a brief description, perhaps in a footnote, about the use of the geometric standard deviation, and why this rather than some other statistical measure of variability was chosen by RAC?

In general, the distributions were either described in the literature as lognormal or the distributions created from the available data fit best to a lognormal; the statistical measures selected to best describe this distribution were the geometric mean (median) and geometric standard deviation.

Page vii, first and second full paragraphs. I strongly endorse RAC's approach to use actual air and wind data. In particular, if there is any suggestion that RAC should revert to the 1996 value for mass loading, I urge that RAC hold firm in its choice.

We plan to stick to this approach.

Editorial. Page 1, 2nd and 3rd paragraph. Some language should be added to distinguish the Monte Carlo feature in the new version of RESRAD from the Monte Carlo interface developed by RAC, just to avoid confusing non-specialist readers.

We will incorporate this enhancement.

Editorial. Page 1, 4th paragraph, 4th sentence. Can some qualifier be put on "large", say, XX% change? Alternately, could there be a definition in the next sentence, where sensitive, limited sensitivity and no sensitivity are listed?

We have incorporated qualifiers into these sections of the report.

Editorial, and perhaps more than that. Pages 2-3, Differences between... This section (especially the first paragraph) needs some clarification and elaboration, if for no other reason than the roughly 5-6 fold increases in the soil action levels for plutonium shown in Table 1, which leap out at the reader. First, aren't there *two* changes (not one) between the two versions, the change in the air concentration and the addition of wind speed? In the text, can you provide some perspective on the relative importance of the two? Also, is "adjusted" a better choice than

"altered"? Should "overly" be inserted before "conservative"? I suggest that RAC take a fresh look at this entire section with the goal of making it more explicit.

This section of the report, in particular, has spurred a great deal of discussion and even controversy. A comparison of the two versions of RESRAD used during this study was included in this report only as a means of illustration. We intended to show that the resuspension mechanism (the single change we refer to) in the more recent version is significantly different than in the previous version. What we propose because of this difference is a resuspension calculation based on actual site measurements as opposed to this generic, and generally unsatisfactory when viewed from an output perspective, resuspension calculation. We plan to clarify and make explicit the point of this section in the final version of this report.

Editorial, page 3. In my copy of the draft, there is a speck of black that on first reading turned 1088 into 1.088. I trust it was added by the copy machine, and does not exist on the original.

The copy machine did add the speck of black; the value on page 3 should read 1088, not 1.088.

Editorial. Page 5, 1st paragraph, last sentence. Substitute "believe" for "feel".

We will make this adjustment.

Editorial. Page 5, 2nd paragraph. RAC selected 0.03 meters to maintain consistency with *which* definition, the one for soil mixing layer or thickness of the contaminated zone? And why is RAC comfortable being consistent with inconsistent definitions? Is the phrase "surface or resuspendible soils" the best one available?

As mentioned in the response to comments from Reviewer A, it is likely that the discussion in this section is not adequate to describe what we intended with the selection of 3 cm as the depth of resuspendible soil. We will adjust this discussion to be more consistent with our intent, as described in the response to Reviewer A.

Editorial. Page 5, 2nd paragraph and 5th paragraph. Perhaps there should be a little more explanation of the use of 0.03 meters for depth of mixing layer versus 0.2 meters for thickness of the contaminated zone.

As a result of the significant number of comments about the above two quantities, we will look at reworking the section which explains the use of the two values.

Editorial. Page 5, 3rd paragraph. Would RAC be comfortable adding "very" before "conservative" in the last sentence of this paragraph?

Several reviewers had a comment about this quantity for indoor dust filtration. This was the quantity used in the previous analysis, and RAC saw no reason to change the value for the present analysis. We plan to explore the use of a distribution for this value.

Editorial, page 7, 3rd-4th lines. The exponent got bumped down a line.

We will fix this for the final version.

Editorial. Page 9, Table 3 and following paragraph. "(DCF)" should be added to the heading on the table, and "f_1" is not defined either in a footnote to the table or in the text.

We will make these changes.

Editorial. Page 11. I recommend that the order of the parameters and the two columns be identical to those in Table ES-1 on page vi. Also, would it help to break this mega-table into a set of tables? In particular, for the parameters not exhibiting sensitivity, should there be one table for the ones where DOE and RAC values are different, and a second one where they are identical? Finally, shouldn't "not" be capitalized in the heading of the last group of parameters?

We will put the parameters in the same order in the two tables. We have struggled with the readability of this table, and will continue to make adjustments to make the table easier to read.

Editorial, and perhaps a bit more. Page 13, first paragraph. I suggest language be added explaining the utility of including the "bounding level, screening calculation" for the one scenario, including stating whether it is meant to provide an upper bound or conservative estimate.

The bounding level, screening calculation is important primarily for the sake of completeness in the review. We recognize that we cannot make a detailed quantitative evaluation of the dose from the groundwater pathway, but we would like to provide perspective and perhaps some encouragement to explore future work in this area.

Page 15. If possible, could RAC be a little more descriptive of the type of study it believes necessary, and in addition, provide recommendations on how RAC's own final results (whatever they may be) should be re-visited when such work by others is complete? This might include running sensitivity studies, for example.

The Task 5 report might be a better place to provide recommendations on how our final results might be revisited at a later date. Such recommendations will be incorporated in that report.

Editorial. Page 17, 3rd full paragraph, last sentence. Substitute "The" for "A".

We will make this change.

Editorial. Page 20, 2nd and 3rd paragraphs. Did RAC "define" the model, or did RAC "build", "develop", or "construct" the model?

We will include more appropriate wording in these locations

Editorial. Page 22, 2nd complete paragraph, and page 23, legend for Figure 2. Was Litaor's contribution in this regard so great that it justifies a complete name, the only individual so honored in the entire report? Also, more facetiously, does the name Iggy generate a high degree of technical confidence in the average reader? (Even RAC rejects much of Iggy's data in the 3rd paragraph on page 22.) I suggest either just using the last name, or M. I. Litaor.

We will use M.I. Litaor to refer to this individual the first time.

Editorial. Page 29, 2nd full paragraph. Insert "and" before "annual" in the next to last sentence.

We will incorporate this change.

Editorial, Page 31 ff. Using "current" to describe the 1996 scenarios bothers me somewhat, especially since "current" is also used to qualify the onsite worker scenario. Labeling them as "1996 scenarios" also doesn't seem quite proper, though strictly speaking it would be correct to do so. Since RAC has four and the 1996 effort had three, perhaps the editorial solution is to describe the origin of the three in one place, as in the 4th paragraph on page 31, and then later identify them as the "three scenarios" or "the DOE/EPA/CDPHE scenarios" both in the text and in tables (such as Table 10). RAC's can be identified as the "four scenarios" or the "RAC scenarios", as appropriate.

We appreciate this comment, and will do everything we can to clarify the language within the report, making it clear at all times to which project and which scenarios we are referring.

Editorial. Page 40, 3rd complete paragraph. The exponent on "d" should be –1, as it should also be for "y".

We will make this change.

Reviewer D

This is a well-conceived and useful draft report, as was the Task 2 Report by the same authors. Prior to commenting on that earlier report, this reviewer raised a number of concerns regarding the assumptions underlying the DOE/EPA/CDPHE application of EPA's 15/85 mrem/y dose criteria and their choice of exposure scenarios for implementing those criteria via soil action levels, including the selection of parameters characterizing the individuals exposed. This Task 3 draft begins to address many of those concerns. At the risk of boring the reader of these comments, and since paper is cheap, I repeat here the basis for those concerns before commenting on how this report addresses them:

1. Misuse of EPA's draft 85 mrem/y criterion.

This criterion was proposed to assure protection during unanticipated failure of institutional controls only. It was not meant for planned land uses in the distant future when controls are assumed to no longer exist. EPA requires review of institutional controls no less often than every five years as long as they are needed to meet 15 mrem/y (40 CFR Part 300.430(f)(4)(ii)). Failures are expected to be of short duration and corrected when identified. In EPA's current regulations (OSWER Directive No. 9200.4-18; August 1997) the 85 mrem/y criterion has been dropped -- it is assumed unnecessary under the periodic review requirement.

It appears that reasonable assurance of effective long-term institutional control at Rocky flats for the duration of the hazard is not now available and is, in fact, probably not possible. Accordingly, cleanup of the entire site to 15 mrem/y now, without reliance on controls, is, realistically, likely to be needed. The choice of exposure scenarios for the Tier I Action Levels for the so-called "buffer" and "industrial" areas is affected, as well as for areas *outside* the buffer areas, since these locations clearly must meet 15 mrem/y under unrestricted use in any case, and the action levels for the immediately adjacent buffer area, under the existing proposal, permit significantly higher levels.

We will be completing calculations using both the 15 and 85 mrem y^{-1} criteria, presenting these values to the panel, and allowing them to make recommendations based on these results. The panel could likely use this reviewer's comments to expand its understanding of this topic.

The draft report proposes two new exposure scenarios that go a long way toward providing the basis for satisfying the above needs: the "current site industrial worker" and the "resident rancher." With respect to the industrial worker scenario, I assume that the choice of 60% of time spent outdoors reflects the seasonal nature of outside work and that this scenario could therefore reflect a grounds maintenance worker. However, the assumption of no onsite drinking water does not appear justified for such an individual.

The groundwater/drinking water calculations will be completed only for the residential rancher as a bounding level, screening calculation to provide some perspective on the potential for dose from this pathway.

There are more serious problems with the resident rancher scenario. I assume that it was considered more reasonable to posit a resident rancher than a rural resident based on current land uses (no explanation is given in the report). However, given the present the rate of expansion of populations in the Denver area and the extremely long duration of this hazard, that choice would appear to be extremely difficult to justify over the long term, and no justification is provided in this report. It also is not clear what the justification is for selecting only 40% time outdoors for a resident rancher (rather than 60%, as in the case of an industrial worker), nor is it clear why this scenario is restricted to locations east of the 903 area (instead of including that area). The report needs to modify these assumptions or provide a convincing rational in support of them. (See also the comments below on the definition of the RME individual required to be protected under CERCLA. It would not take many rural residents to constitute their designation as the RME individual.) Comments on the usefulness of the infant and child scenarios are provided later. Incidently, the headings "nonrestrictive" and "restrictive" appear to be reversed in Table 10.

Thanks to the reviewer for noting the reversal of the heading in Table 10; this will be changed in the final report. These scenarios were selected after many long discussions with the panel and were approved by them in May. The scenarios were designed to address not only what we know about the possible future at Rocky Flats, but also what we do not and can never know about events that have not occurred yet. We will elaborate on our discussion on time indoors and outdoors for the scenarios in the final report.

2. Inadequate Exposure Scenarios:

My previous comments on this topic were: "Under CERCLA, the choice of exposure scenarios is intended to assure protection of the "*Reasonably Maximum Exposed*" (RME) individual. This is not the same as the *average* member of the affected population, nor is it the *most* exposed individual. EPA has devoted considerable effort to clarifying this admittedly elusive concept. The following quotes are typical of EPA guidance:

"'...actions at Superfund sites should be based on an estimate of the reasonable maximum exposure (RME) expected to occur under both current and future land use conditions. The reasonable maximum exposure is defined here as the highest exposure that is reasonably expected to occur at the site... The intent of the RME is to estimate a conservative exposure case (i.e., well above the average) that is still within the range of possible exposures.' ("Risk Assessment Guidance for Superfund, Volume 1, Human Health Evaluation Manual (Part A) Interim Final," EPA-502/1-88-020.)

"'The high-end of the risk distribution is, conceptually, above the 90th percentile of the actual (either measured or estimated) distribution. The conceptual range is not meant to precisely define the limits of this descriptor, but should be used by the assessor as a target range for characterizing "high-end" risk.' ("Guidance on Risk Characterization for Risk Managers and Risk Assessors," Memo from F. Henry Habicht II, Deputy Administrator, EPA, to Assistant Administrators and Regional Administrators, February 26, 1992.

"A number of the choices in the DOE report do not appear to meet these criteria, but instead are more reflective of average populations or behavior of individuals. For example, the office worker chosen for the industrial area scenario reflects the average worker for the assumed use of the area as office buildings. However, an RME individual at such a site would be a maintenance worker, who takes care of the assumed "well-maintained landscaping" (DOE report, p. 6-16). It is also not at all clear that the "industrial" area would be exclusively used, for office buildings for the duration of the hazard. Given the relatively remote location of the Rocky Flats site, it appears optimistic to assume that use of this area would be so limited. A more realistic scenario would envision more traditional industrial uses, such as lumber yards, light industry, or even scrap yards. Under these uses the office worker scenario becomes untenable as the basis for deriving soil action levels.

"A similar difficulty arises for some of the choices of exposure parameters for the individual scenarios. For example, the exclusion of ground and surface water in the rural residential scenario does not appear to reflect the RME individual. What assurance is there that less than 10% of individuals would not avail themselves of existing ground or surface water at any point in time during the next 1000 years? The existing ground water appears adequate for subsistence living, and quite adequate for use for limited irrigation, as for a family garden. If non-use of ground or surface water is an assumption, rather than a condition assured through an institutional control, it is not an appropriate element of the exposure scenario. (In any case, in the scenario for the 85 mrem/y criterion, when institutional control is assumed to be absent, non-use clearly should not be assumed.). Other parameters that warrant examination are the assumption of no contamination, now or in the future, below 15 cm, when plant roots are assumed to penetrate to 90 cm, and the degree of retention of mass loading for foliage assumed for this semi-arid area in the rural residential scenario; the assumption of no use of surface or ground water and the time limitations on annual usage by the RME individual in the buffer zones; and, for all the scenarios, the blanket assumption of Class Y solubility for plutonium under all pathway conditions."

The present report addresses many of these problems. Importantly, in addition to the new scenarios noted above, it adopts the 95% breathing rates, in conformance with the RME individual, and includes (to the extent feasible) bounding doses due to ground water for a number of the scenarios. There are, however, some remaining problems.

The report adopts, as plausible, all three scenarios outlined in the Rocky Flats Cleanup Agreement. This is not reasonable, since these do not satisfy the CERCLA criteria outlined above: The "office worker" is clearly not an RME individual; ground water intake is still not considered for the "resident;" and, at least according to Table 10, both the "resident" and the "open space user" spend 100% of their time indoors!

We have accepted the DOE scenarios as part of the total scenario analysis for this project. The results of the calculations will be provided to the panel, and the panel will have a chance to make recommendations based on the results of all of the scenario calculations.

It is important to point out that although the resident and open space user spend their time indoors, the air concentration indoors has been set to be equal to the air concentration outdoors.

The infant and child scenarios omit onsite drinking water (infant formula is made with water, and children drink the same water as their parents!).

The drinking water analysis will only be presented as a bounding level, screening calculation to present the potential for dose from this pathway as a means of encouraging further research in this area.

The report omits irrigation water from the child scenario, but include it for their parents (children eat most of the same produce that their parents eat).

Again, this calculation is not meant to be quantitative in terms of providing boundaries for dose, but rather to present screening results.

Finally, I have a major conceptual problem with the use of infant and child based scenarios. They misuse the annual dose criterion by artificially inflating its effect. The basis for the dose limit is lifetime risk, which already includes the risks due to exposure during infancy and childhood. The annual dose criterion is a useful surrogate for lifetime risk only if it is applied to standard man, and was never intended to limit annual risk to a uniform value for any age individual. (If that were true, permissible annual doses for senior citizens would be extremely large.) I strongly recommend dropping these scenarios.

These scenarios are very important to the panel, as a means of presenting results that are meaningful to all possible recipients of dose. For parents living in the vicinity of the plant, this means that their children need to be assured of protection. We will take this reviewer's comments to heart in our presentation of the results for these scenarios.

Other Comments on the Task 3 Report.

The report should at least comment on the subject of co-variance, in the context of the use of single-parameter analysis (p. v).

Co-variance suggests the possible correlation of parameters. Although a single-parameter analysis ignores possibilities of correlation, there is some possibility of this, which we did consider while completing the analysis. We will consider adding some text that relates to this.

It is not clear that the use of existing actual air monitoring data can approximate future land use conditions that do not now exist - e.g., agricultural use under drought conditions (witness current mid-Atlantic agricultural regions). I suspect that such a procedure would underestimate inhalation doses due to resuspension (p. vii). In this regard (the degree of conservatism appropriate), to what extent can we predict the effects of climate over a 1000-year period on enhancement of resuspension?

We intend to present enhancement factors that simulate these types of conditions and make the resuspension pathway more broadly applicable to the range of possible future conditions at Rocky Flats.

Endorse the proposed use of current estimates of fruits, vegetables, and grains, especially in view of current dietary trends (p. ix).

The choices for parameters with limited sensitivity appear logical (pp. 4-5).

The discussion of the gamma shielding factor represents an improvement (p. 6).

The treatment of ground water ingestion (pp. 13-15) confirms that more work is needed on this potentially important pathway, especially with respect to colloidal transport of americium. The observation of Honeyman that study conditions (increased well pumping) in the Kersting et al. work may have enhanced colliodal concentrations is provocative – that is just what extensive ground water use would do.

We thank this reviewer for all of the above comments.

Figure 4 suggests that some supplementing of the spatial soil model may be desirable to accommodate the higher measured values at the bottom of the figure, which appear to be an order of magnitude higher than the model predicts.

We continue to review the spatial soil model for improvements through the production of the Task 5 report.

Would it make sense to use the 95% value for soil ingestion, but multiply it by seasonal and weather-based soil availability factors (e.g., 0.5 for frozen or snow covered, and 0.7 for rainy weather during the balance of the year, or 0.73x0.5x0.7=0.26)?

This parameter has been extensively discussed by the panel, and the values presented represent the final conclusions regarding this parameter.

Minor comments.

1. Is there any way to provide for the possibility of colloidal transport in the uncertainty analysis?

Not within the boundaries of our screening only analysis.

2. Has retention of foliar deposition been evaluated for the semi-arid conditions at Rocky Flats?

We will look into availability of data of this type.

Reviewer E

General Comments

1. The report lacks a complete overview of the sensitivity analysis performed. The following two questions are left unanswered.

a. Why was the sensitivity analysis limited to site-related parameters? For the convenience of the reader, the universe of input parameters to RESRAD should be categorized and it should be clearly stated in the introduction and executive summary which categories of parameters were evaluated in the sensitivity analysis, which were not, and why. For example, two obvious categories are:

site -related (or environmental fate and transport) parameters (e.g., those listed in Table 4), and

exposure-related (or scenario) parameters (e.g., those listed in Table 10).

The sensitivity analysis was limited to the site-related parameter because only these parameters will be treated stochastically in the soil action level analysis.

Although the RFP and RAC's proposal did not limit the sensitivity analysis to site-related parameters, that is what apparently was done. There may be good reasons for this. They should be made explicit.

The sensitivity analysis was limited to site-related parameters as agreed upon by the panel. Scenario-related parameters represent human characteristics or habits. For our hypothetical individuals, we assume that we understand the characteristics of a specific individual, but present a variety of scenarios so that many different types of individuals are represented.

b. Which exposure scenarios were evaluated in the sensitivity analysis? If all scenarios were evaluated, were the results consistent for all (i.e. were the same parameters sensitive for all scenarios? (For example, p. vi, par. 1 implies that Kd was only sensitive for the rancher scenario where groundwater was considered as a source of drinking water. Is this the case or was Kd important for all scenarios?). It seems that there would be a way to create a table illustrating (qualitatively or quantitatively) which parameters were important for which scenarios to provide a summary answer this question.

No exposure scenarios were evaluated in the sensitivity analysis. Each scenario represents a single individual with unique physical and behavioral characteristics. These characteristics include variables correlated with dose, such as average breathing rate or dietary habits. As explained in the report, we used a wide range of references for information on these parameters. Then we generated a distribution of values and sampled from the distribution, using Monte Carlo techniques. This process considered the available studies equally. We selected a certain percentile from that distribution for each scenario. Once a parameter value was selected from our distribution of values for use in the scenario, the scenarios were considered fixed.

2. The ultimate purpose of the current analysis, as I understand it is to develop revised soil action levels for RFETS, where, using RAC's words, a radionuclide "soil action level is a concentration of radionuclide in the soil established to protect people from receiving radiation above a set limit "(p.v). I assume radionuclide soil action levels (RSALS) will be used as soil remediation goals at RFETS. Yet, it seems that RAC has focussed a lot of effort on setting up a *baseline* risk assessment by developing contours of actual contamination levels to specify initial contamination concentrations and areas for use in RESRAD and using site data to develop relationships between contaminant concentrations in air and soil for use in the resuspension calculations. I agree that this approach will make, as RAC states "the calculation of dose more meaningful"(p.viii).

However, it is the dose due to due to current contamination levels that will be calculated. I'll call it the baseline dose, here. I think that RAC's proposed analysis makes the baseline dose more meaningful, but is not feasible for calculating RSALS. To develop RSALs, one needs a different analysis, the purpose of which is to assure that the dose at the RSAL (or post-remediation radionuclide concentration) is less than or equal to the target dose with some level of confidence.

I have some questions about whether RAC's approach outlined in Task 3 will lead to meaningful RSALs in Task 5. RAC makes the claim that their procedure to calculate resuspension parameters (described under the heading "Mass Loading Factor" p. 27) will be used to " estimate annual average plutonium air concentration at any location at or near the site"(p.28, par.. 4) They go on to say that they "*may* [emphasis added] also estimate plutonium air concentrations based on the assumption of reduced soil concentrations that simulate the results of remediation" (p. 28, par. 3). Isn't the latter the point of the whole analysis--which is to develop RSALS? Additionally, even if the relationship between current soil and air concentrations is elucidated for the baseline risk assessment, what assurance is there that the same realtionship would be appropriate for a remediated site?

RAC justifies their approach for calculating the resuspension parameters based on the fact that " air concentrations in the domain of a scenario depend not only on soil contamination within that domain, but also on soil contamination throughout a larger region" (p.28 par. 4). I do not question that this is an important consideration in a baseline risk assessment. However, I wonder how this can be accounted for in the development of RSALs since you would not know before a remediation effort exactly what the contaminant concentrations in soil would be following the remediation effort . It seems to me that at best you have to assume that the entire area is uniformly contaminated at the RSAL (since theoretically that would be the goal of the remediation effort). I suggest that the original approach in the DOE/EPA/CDPHE (1996) analysis for setting RSALs where it was assumed that there is a large area with uniform contaminant concentration.

The bottom line is this: It seems to me that different methodologies and inputs are called for in calculating baseline risk and RSALs. I think RAC needs to be very clear about the methodologies and inputs they are using for each. In addition, the panel needs to be clear about which analyses it wants RAC to perform.

While this reviewer may not understand the fundamentals of the approach we are taking here, we want to assure the reviewer and the panel that this analysis will produce the desired results, as will be shown in Task 5.

Specific page-by-page comments:

1. Contents. I suggest some modified headings that reflect my general comment no. 1. 'SENSITIVITY ANALYSIS' and 'UNCERTAINTY DISTRIBUTIONS' should be secondary to a heading like 'SITE-RELATED PARAMETERS'. Similarly, 'SCENARIOS' should be renamed to something like 'SCENARIO-RELATED PARAMETERS' (this section should include a brief introductory statement that points how that scenaio-related parameters will be treated deterministically in the analysis).

At the very least, we will include a statement about how scenario-related parameters were treated deterministically.

2. p.v , last par.. Suggested revision for second sentence which as it reads now appears to confuse uncertainty and variability.
" The probability distribution functions describe the uncertainty in the parameter values that arises due to ....."

We will carefully consider this suggestion and look at revising this sentence.

3. p.v. Regarding the use of the term 'distribution coefficient'. At least at first --in the exec summary and intro-- be more specific. Replace with 'soil-water equilibrium distribution coefficient'. In general in environmental fate and transport modeling, there are other types of distribution coefficients.

Good suggestion – we will make this adjustment.

4. p.vii. par. 1. Start with "The term 'mass loading' is used in this analysis as..." Here, too, there is no standard definition for 'mass loading' in environmental fate and transport modeling. To avoid confusion, just be clear about your definition for use in this analysis.

We will make this adjustment.

5.. p. vii, last par.. Bullet the list of five less sensitive parameters to make it easier on the reader.

We will make this change.

6. p. ix. before last par.. Make it clear that deterministic values will be used for scenario-related parameters in the assessment.

We will make this clear in the final report.

7. p.1, par. 3. It is not clear at this point (and it should be) why RAC has developed a Monte Carlo interface for RESRAD when in the previous paragraph it says RESRAD has one already.

The interface built into RESRAD that was used in the sensitivity analysis was built on Monte Carlo principles, but accomplishes only a single-parameter sensitivity analysis. There is an additional interface built in to RESRAD that supposedly creates uncertainty distributions, but which the authors of this report had no luck getting to run. Nonetheless, it is important for *RAC* to develop their own Monte Carlo analysis for two reasons. 1) It is a contract requirement that we build a Monte Carlo interface, and 2) We needed to build our own module to incorporate the alternate calculation of resuspension.

8. p. 4 par. 3. It seems more appropriate to have performed the sensitivity analysis using the total possible range of values for all the parameters rather than to have varied the parameters by a factor of 10 in either direction.

The analysis could have proceeded in many different directions, but we chose one and stuck to it.

9. p. 9 1st par. under 'Remaining parameters', 1st bullet. Isn't K_d a saturated zone parameter? Perhaps this bullet item needs to be more specific or needs to specifically exclude K_d.

We will make a change that will exclude K_d from this list.

9. p. 11 Table 4. Most, but not all of the information from Table ES-1 is repeated here under 'sensitive parameters'. Table 4 should be at least as complete as Table ES-1 or it should just refer to Table ES-1.

We have had another comment on this, and will make the appropriate changes.

10. p.18, 2nd par. under Table 6, last sentence. Be more specific about what you mean by the 'midpoints of the Kd values from Table 5'.

Thank you for this comment. We will attempt to be clearer in the final version of the report.

11. p. 20 par. 2. This paragraph starts with "To resolve this problem...". It is unclear how this resolves the problem.

We solve the problem presented by RESRAD (homogeneity of contamination required) by incorporating our own spatial soil model that allows heterogeneity of soil contamination to exist.

12.. p. 31 2nd par., last sentence. Makes no sense. Re-read. Re-word.

We will work to clarify our view of scenarios.

13. p.31 4th par., last sentence. Redundant with 1st sentence.

We have had several comments of this sentence from good, careful reviewers. We thank this reviewer for this comment and will change this sentence.

14. p. 33 Table 10. 'Soil Ingestion' in first column should be in units of g/d. Otherwise it looks like 0.2 g/ 365d which is 0.0005 g/d. With this change, might have to clarify the wording under the open space scenario.

We will work to make this section of the table more readily understandable.

15. p.33 Table 10. Why is there 'NA' entered for drinking water ingestion under the infant of rancher and child of rancher. If the adult rancher drinks the well water, why don't the infant and child?

We are conducting a groundwater/drinking water analysis only as a means of presenting the results of screening calculations. We have agreed to include the pathway for only one scenario, the residential rancher.

16. p.33 Table 10. Why is there 'NA' entered for the 'Fruits. vegetables and grain consumption' of the 'Infant of rancher'. p. 40 indicates that this value should be entered as 200.

16. p.33 Table 10. Why is there 'NA' entered for the 'Leafy vegetables' of the 'Infant of rancher'. p. 40 indicates that this value should be entered as 26.

There appears to be a typo. We will make the table and the text consistent.

17. p.37 1st par.. second to last sentence. Give the units on the 'geometric mean of 0.2'.

Thank you – we will make this change.

References

US DOE, US EPA, CDPHE (1996) Action Levels for Radionuclides in Soils for the Rocky Flats Cleanup Agreement Final.(October 31, 1996).

PANEL COMMENTS

Victor Holm

I was impressed with your Task 3 report. First it was well organized and very readable. Your early decision to concentrate on a few parameters that are most sensitive has served to focus attention and prevent endless debate over matters that have little or no practical value. I was especially impressed by the way you integrated the many previous studies at Rocky Flats into the work, especially the sections on Area of the Contaminated Zone on pages 19-27 and the discussion of the Distribution Coefficient. Sometime important data affecting a parameter are discussed in a different section, but short of repeating the data in both sections, I don't see a solution.

The discussion of scenarios seems to fit better here than in Task 2 and the discussion is much more complete than in the draft.

I, along with Bob Kanick were instrumental in selecting a quantitative risk assessment for this study. The reason for this was the expressed concern by several members of the panel that safety factors be incorporated in the final result. We understood that if safety factors were incorporated individually in each parameter there would be no way to evaluate what the final safety factor might be. Secondly many of the parameters did not lend themselves easily to quantified safety factors. Instead what we hoped for was a realistic estimate of the distribution of the probability of doses. The panel, with help from the contractor, could then set a safety factor by selecting a given probability, say 90%. As you are aware, I was uncomfortable with some of the behavioral parameters RAC selected. It was explained that the applicable guidance suggested using the 95% value for the behavioral parameters. While NUREG 1549 does recommend this approach for deterministic evaluations it specifically recommends actual distributions be used for probabilistic studies. At the time we discussed scenarios, I was assured that for the environmental parameters, the best scientific estimate would be used without additional safety factors. I was dismayed to see that for some of these parameters you made statements like "We feel that the use of this conservative value is reasonable, and will not be changed" or "while this is a conservative assumption, RAC will not change this value for our independent calculation because of the recognized importance of the inhalation pathway". In a quantitative risk assessment adding safety factors like these only serves to bias the result. To place safety factors on only the most important variables simply says if we are going to bias the result lets really bias it. If safety factors are to placed on the environmental parameters the resulting distribution of the doses will be biased, worse it will not be possible to quantify this bias. I would have difficulty in supporting any value other than the median from such a biased distribution of doses. What is really unfortunate is that for one of the variables that had the safety factor added, cover depth, the site data clearly shows that the correct value is zero therefore no safety factor is required. In the other cases there is ample scientific evidence for a site specific value therefore a safety factor is not required. The statements are therefore gratuitous but nevertheless do great harm to the study. They will tend to confuse the scientific reader and will provide powerful arguments with which to discredit the study. I ask that they be deleted.

We will delete any comments of this type. We appreciate this comment, and we address specific details below with regard to each individual parameter.

There are three specific parameters that I would like for you to review and comment on.

Indoor Dust Filtration

There is nearly a full page discussion on the External Gamma Shielding Factor, a parameter RAC admits has little effect on the RSAL, but only a short paragraph on the Internal Dust Shielding factor which RAC considers important. More disturbing is the RAC's justification for using the highest value: "While this is a conservative assumption, RAC will not change this value ... because of the recognized importance of the inhalation pathway". Are we to assume that the value chosen depends on it's importance to the calculation. How is this any better than a screening analysis. There would perhaps be some justification for the value used if a scientific value was not available. A casual examination of the literature revealed several studies that could be considered.

The RESRAD default is 0.4 following Alzona et. al. (1979). Harkonson and Kirchner (1996) in their critique of the RFCA RSAL values cited Romney and Wallace (1976) as supporting a value of 0.10. NUREG CR-5512 cites a IAEA publication as finding a substantial reduction in indoor dust levels vs outdoor levels. Schmel (1980) was also cited; he studied dust levels during various indoor activities including vigorous sweeping. A NRC draft report (1998) compares the approach in RESRAD to DandD. RESRAD simply scales the outdoor dust level while in DandD indoor dust levels are independent of the outdoor levels. This is following studies that show that most of the indoor dust levels are derived from indoor sources. The default indoor dust mass loading attributed to outdoor sources in DandD is 2.82x10-6 which in most cases is much less than the outdoor level. Lastly common sense would suppose that indoor dust levels are less than outdoor levels especially during the winter when the house is closed to outside ventilation.

After reviewing these studies I suggest that a value of 1.0 is not supported by the studies even at the screening level. I would suggest a normal distribution centered on 0.4 with a standard deviation of .15 truncated by 0.0 and 1.0.

We greatly appreciate these comments. They were quite helpful, and have caused us to take a second look at the indoor dust filtration.

Admittedly, the indoor dust filtration factor has the next greatest impact on the outcome of the calculation than any other parameter mentioned outside of the most sensitive parameters. It would also have a much greater impact on the calculation were the inhalation pathway in RESRAD V. 5.82 not minimized like it is, and this parameter will likely have an important effect on the final results of the *RAC* calculation.

Leaving the parameter at its DOE/EPA/CHPHE defined value was more a resources decision than anything else. We would like to spend a great deal of time defining what this parameter might be for different parts of the country, and specifically for Rocky Flats. There is a great deal of evidence that supports the use of a distribution to represent this value. We were at a place in the production of this report where the resources were better spent developing other parameters.

The comments on this parameter value, but particularly the comment from this reviewer, encourage us to look again at a possible distribution of values for this parameter. We feel that under unknown conditions, 1.0 is still a reasonable upper bound for this parameter. We don't yet have a feel for what a lower bound or median value might be, but

we have thought that an appropriate shape for the distribution might be skewed toward the higher end of the possible range (with the majority of the probability centered toward the high end).

We will continue to explore this parameter for the final version of the Task 3 report.

Area of Contaminated Zone

I had difficulty following your discussion of why the area of the contaminated zone is uncertain. You are correct that given the present contamination it is difficult to assign an area that is both homogeneous and includes the entire contaminated area. Your approach to the problem is novel and I believe it reveals many interesting insights into the origin and fate of the contamination coming from the 903 pad. As an estimate of the area of contamination I am less impressed. RESRAD assumes that the receptor is located at the downwind edge of the contamination. Given this assumption if the area of contamination includes large areas below the RSAL the dose to the receptor would be diluted and could result in estimating a lower than actual dose. If instead you think in terms of the maximum exposure to the receptor after the cleanup levels are met the problem is much easier. The cleanup should result in a large homogeneous area at a level below the RSAL. A problem with this approach is it is recursive, how do you find the area to be remediated before you determine the RSAL. As with many recursive problems this one converges. At least at Rocky Flats the area of contamination drops off rapidly with increasing radionuclide level. As a first assumption we could use the area for the RFCA Tier II residential Pu RSAL's which is 115 pCi/g. The area would then be about 120,000 m2 . I would use this value as the mean of a normal distribution with STD of 25,000.

We do not plan to use the RESRAD evaluation of receptor location and thus we will not use the RESRAD area of contamination. Because we are convinced that it is more meaningful to assess resuspension through use of the existing profile of contamination combined with the air concentration measurements, we need this profile. We hope this entire approach will become clearer through Task 5.

Distribution Coefficients

The groundwater pathway in RESRAD presents a dilemma to the modeler. If the pathway is to show any dose within the 1000 year modeling time the radionuclides must be mobile. If they are mobile then RESRAD shows they are rapidly leached from the soil resulting a decreased inhalation dose. In reality both may be contributors to the dose but the single parameter in RESRAD does not permit modeling this possibility. RAC has chosen a low value of Kd for Pu and Am based on the work of Dames and Moore (1984) in order to evaluate the groundwater pathway. The downside of this approach is it postulates a rapid decrease in inhalation dose. The distribution coefficient is normally thought of as a measure of the chemical leaching and movement of the soluble form of a radionuclide. More generally it can be thought of as a measure of mobility by any process including chemical, physical or biologic processes. The recent work of the Actinide Studies Group summarized in the present report on pages 7 and 8, indicates that chemical mobility probably is not important. RAC's excellent summary on pages 20 thru 25 of the present report presents a good basis for assuming that mobility in the top 30 cm of soil is controlled by a combination of biological and physical factors. Below 30 cm these

processes seems to slow down. Litaor's new paper contains data to support that nearly all of the Pu transport at Rocky Flats occurs through flow of discrete particles or possibly colloids along localized shallow subsurface flow. This flow only occurs when the top several meters of soil have become saturated. He estimates that these conditions occurs about once every fifteen years. Under these conditions the movement is lateral and follows topography. The RESRAD groundwater model is completely useless to handle these conditions. Based of the best data available the Actinide Studies Group has made a preliminary estimate that the Kd is between 10,000 and 100,000 with 20,000 being the most likely value. I would recommend that RAC examine the groundwater pathway separately from the base case. For the base case a lognormal distribution with a geometric mean of 20,000 could be used for Pu. I don't have a suggestion for Am but it is probably over 10,000 cm3/g.

Lateral movement of actinides may be important in fact it may determine the cleanup levels. I am not suggesting eliminating the pathway on the contrary it is too important to use a false and simplistic model like RESRAD. Your preliminary work shows groundwater contamination becoming a problem in several hundred years; I believe it is a problem today. I suggest using a qualitative model like Litaor's to give some early warning of what to expect.

Based on the comments of this and other reviewers, we will evaluate the residential rancher both with and without the groundwater pathway, to provide some indication of the impact that this pathway might have on dose. It is true, as this reviewer points out, that the groundwater pathway within RESRAD presents a dilemma. It is clear that within the context of this study, the details of this pathway cannot be worked out, but can be at least qualitatively evaluated for direction of future studies.

We are examining the referred to document to better assess a distribution for K_d.

References:

Alzona J. et. al. 1979, "Indoor-Outdoor Relationships for Airborne Particulate Matter of Outdoor Origin", Atmospheric Environment 13:55-60.

Dames and Moore 1984. De Minimus Water Impacts Analysis Methodology. NRC study

Harkonson T. E. and Kirchner T. B. (1996) Oral report to the RFCAB Spt 9, 1996

IAEA, 1970, Monitoring Radioactive Contamination on Surfaces. Technical report Number 120

Litaor I., Barth G., Litus G. 1999, The Hydro-geochemistry of Actinides in the Soil of Rocky Flats, Colorado, manuscript.

NRC Draft Report 1998, Comparison of the Models and Assumptions used in the DandD 1.0, RESRAD 5.61 and RESRAD-Build Computer Codes with Respect to the Resident Farmer and the Industrial Occupant Scenarios provided in NUREG/CR-5512.

Romney and Wallace (1976) I don't have the reference.

Schmel G. A. 1980, Particle Resuspension: A Review, Environ. Int. 4:107-127

Joe Goldfield

Portions of the task three report are very troubling. One cited soil action level, resulting from the application of RESRAD to Rocky Flats open space, is 53,120 pCi/g (picocuries of plutonium per gram of soil) well over1,000,000 times as high as the average plutonium background level (0.04 pCi/g).See page three comparing the RESRAD version 5.61 to RESRAD version 5.82. The last column shows an action level of 53,120 pCi/g of soil for the open space and 8351 pCi for a resident with a dose level of 85 mrem/yr.

We intended this presentation to serve only as an illustration of why we have chosen to bypass the resuspension calculation in RESRAD. These values have spurred so much comment that we plan to consider reworking this entire section of the report to include only a discussion of the different versions and not to present tables of values extracted from the versions.

1. The definition of TRU (transuranic waste) that must be sent to WIPP includes materials that contain greater than 100 nCi of plutonium per gram of waste. The cleanup standard for the open space would be over 53 nCi of plutonium per gram of soil (halfway up to the TRU waste designation). Furthermore in accordance with the report on Sampling Protocols, hot spots that may be ten times the cleanup standard (530 nCi Pu/g) would not be cleaned up. Thus areas could contain over five times the lower limit of TRU waste. In accordance with this thinking why would we send anything to WIPP and bury it 2,000 feet underground? If we played our cards right we could spread it around the open space.

Bear in mind that 530 nCi/g is equal to 8413 ng (nanograms of plutonium) (the concentration given in nCi/g must be multiplied by 15.9 to convert to ng of Pu/g) or 8.4 ug (micrograms of plutonium) per gram of soil while the allowable lifetime body burden of a nuclear plant worker is only 1ug. The ingestion or inhalation of a little over a tenth of the soil concentration would exceed the allowable lifetime body burden of a nuclear plant worker.

We did not present the value cited here as a possible soil action level. We presented it only to show how inadequate we believe the resuspension code in RESRAD to be for predicting possible soil action levels and why we believe it to be necessary to prepare our own calculation. We apologize for any confusion this might have caused.

2. On page 39 RAC cites the ingestion of soil at the 95 percentile level as 0.75 g per day. With a level of 8.4 ug of plutonium per gram of soil in the hot spots of the open area--the rate of ingestion would be 6.3 ug/day or 6.3 times the allowable lifetime body burden of a nuclear plant worker. If we place a safety factor of ten or twenty for civilians and children, every day of soil ingestion of hot spots results in ingestion of 60 to 100 times the allowable lifetime plutonum body burden.

Again, the values cited for mass of plutonium in soil are based on results that were presented for illustrative purposes only.

3. Examine the soil action level allowable for residents of the remediated portions of Rocky Flats where the soil action level is 8351 pCi/g which is equal to 8.35 nCi/g (nanocuries per gram

of soil). Converting to nanograms requires multiplication by the factor of 15.9 giving 133 ng of Pu per gram of soil.

RAC states that the data extropolating from soil concentrations to inhalation quantities is meager. My information is also meager. Permit me to use my methods of estimating.

I have seen data that shows that the plutonium in soil is concentrated in the small particle size fraction. Air blowing over the soil would tend to most easily entrain the smaller particle size fraction of the soil. It is reasonable to guess that air borne soil has 3 to 5 times the soil concentration or 400 to 670 ng of plutonium per gram of soil.

If a person breathes 10,000 cu. meters of air per year and the particulate concentration is 90 ug per cubic meter (instead of the 26 discussed in the report), the yearly particulate intake will be 900,000 ug or 0.9 g of soil. That soil would contain 360 to 600 ng of plutonium 9 (30 to 50 ng per month). I have mentioned previously that the allowable lifetime body burden of a nuclear plant worker is 1 ug or 1000 ng (nanograms). Assume a reduction of tenfold for the general population--that allowable body burden would fall to 100 ng. It would take two to three months of residency to exceed the allowable body burden.

This result assumes the average concentration of 8351 pCu/g rather than the probable effect of pockets of contamination that far exceed the average.

Again, the values cited for mass of plutonium in soil are based on results that were presented for illustrative purposes only. We continue to consider revisions to this section of the report to eliminate the ability to make any inappropriate comparisons or calculations with these results, which are not results of this study.

4. The area of the contaminated zone is estimated as 40,000m2. That is 200 meters by 200 meters. That is only 660 feet square. That area is tiny compared to the total plant area which amounts to thousands of acres. The area probably does not include the industrial area which may have ten times the plutonium contamination of the 903 pad. For some reason the discussions of plutonium contamination are restricted to the 903 pad and do not include the industrial area.

This area is not suggested as the area to be used for the new soil action level calculations. This area was used in the previous analysis.

5. I suggest that we have a knowledgeable expert on Rocky Flats meteorology review the meteorological data presented. Gale Biggs in previous reviews took exception to much of the data available at Rocky Flats.

The data presented in this report are from recent (1989-1993) meteorology reports at Rocky Flats. These data have been used in other projects completed at Rocky Flats and we are confident in their ability to predict annual average wind conditions at the site.

6. I have taken exception in the past to the use of 26ug per cubic meter as the particulate concentration in air at Rocky Flats. I understand that that particulate concentration is based on measurements taken by means of high volume PM10 samplers located at Rocky Flats. My reservations are based on the following:

a. PM10 samplers remove 50% of the airborne particulate concentration. Some significant percentage of the material removed is smaller than 10 microns and is therefore in the respirable range.

b. PM10 samplers must be carefully handled to get acceptable data. They must be calibrated so that the exhausted air volume is known accurately. Account must be taken of the pressure buildup on filters and the resultant reduction in flow.

c. The location of the samplers, I surmise, are on the periphery of the property where the site resembles wilderness areas instead of heavily populated and developed areas that may result in the future at Rocky Flats. Our analyses must allow for the foreseeable changes that will occur at Rocky Flats over the next 1,000 years

d. Does the RESRAD program correct the particulate concentration entered into the calculations to reduce the total particulate to account for fractions that may be larger than the respirable sizes? If so, using PM10 results may introduce a double particulate reduction to account for non-respirable size particles.

e. For all the reasons stated and the fact that a consultant reporting to the RFCAB recommended an airborne soil particulate concentration of 90ug, I strongly recommend that the estimated particulate concentration be raised to 90ug per cubic meter.

We plan to derive a resuspension factor/mass loading value from available site-specific data and undertake a calculation of resuspension from this factor independent of the RESRAD calculation.

7. I have not had the time to investigate the subject of breathing rates which I still believe are not being estimated conservatively.

Joel Selbin

I want to see a really detailed explanation of why RESRAD 5.82 yields considerably higher SALs (page 3 and Appendix A) than RESRAD 5.61. The statement on page 2 that the former version of the code used a "conservative treatment" of the very important matter of resuspension is very disconcerting. What other factors are going to have a comparable effect, and in which direction? What happens to SALs at other world sites using the new code?

The comments resulting from the inclusion of this table comparing the results of the two versions of RESRAD are numerous. As stated in response to the previous reviewers' comments, we are considering completely rewriting this section to better reflect the intent of including it in this report.

The documentation that accompanies the newer versions of RESRAD state that the previous treatment of resuspension was conservative and generic. Because the current treatment is still unsatisfactory to *RAC* and appears to produce significantly higher soil action levels and lower doses, we plan to not use the newer version's treatment of uncertainty.

We do not have the resources of the time to review the impact of this code at other sites, and in fact, it is unnecessary given the intent of this presentation: to impress on the panel the importance of the treatment of resuspension that *RAC* is undertaking.

LeRoy Moore

Where do "Relative Biological Effect" numbers for Pu appear in the RSAL calculations? Are they among the inputs and assumptions about which the assumption is made that they do not modify the outcome? If so, I will make a comment on them. If not, when will they be considered? This is an issue about which I pressed hard but to no effect with the government agencies when they adopted the original RSALs.

Relative biological effect is built into risk assessment, which in turn is built into the dose limits provided for this study (15 and 85 mrem y^{-1}). We plan to comment on risk in the Task 5 report in terms of what it means in the context of this study.

p. v, Ex. Sum: about three-fourths down in the opening paragraph a sentence begins: "As a result of public concern about the proposed soil actions levels. . . ." Delete "proposed" and change to read: "soil action levels adopted in Octorber 1996."

We will consider this change to the text.

p. 1: Change opening sentence of Intro to read: "Soil action levels are calculated to identify the concentration of one or more radionuclides in the soil above which remedial action would be required to prevent people from receiving doses above an officially designated level."

We will also consider this change.

pp. 1-3: Why is RAC using RESRAD 5.82 rather than 5.61? My recollection is that at one meeting a couple of months ago RAC presented us with the disturbing info that 5.82's parameters had been so modified that feeding in the same data used by the agencies in setting the original RF RSALs resulted in much higher allowed concentrations of Pu, etc. The text on pp. 2-3 (esp. Table 1 on p. 3) repeats this info. We go from a RSAL for Pu of 1429 pCi/g to one of 8351, which, to put it mildly, is outrageous. I do not recall that the Panel asked RAC to proceed with 5.82. I do recall that there was a request for documentation from DOE of the instructions they gave to Argonne along with their request that RESRAD be updated. Have we received this documentation? Short of getting it and thus understanding why the outcome from calculations is so much higher on the revised RESRAD, I think we should stay with the program used by the agencies initially. Is there any reason we cannot do this?

We used the newer version of RESRAD because, at the outset of this project when we requested source code and documentation, we received source code for Version 5.82. At the end of it all, however, it matters not what version of RESRAD we use as long as it is understood that the resuspension calculation, the only major change in the updated version of the code, will be bypassed for this assessment in favor of a site-specific resuspension model. It is too late in the project to make any changes in the code selected for use, and it is not necessary, given what we plan to do about resuspension.

p. 2, second para. under "Difference between versions": Why use a value for annual mean for Denver area wind speed derived from a National Climatic Data Center report? Isn't there site-

specific data for wind speed at RF? RAC may recall that wind is stronger at RF than in Denver, and that the prevailing wind blows in a different direction. The RF original siting resulted from a mistake about wind, namely, that it was based on readings done in Denver, not at RF itself.

We present this data because it gave us a place to begin our sensitivity analysis for Version 5.82. We plan to use data originating from the Rocky Flats Meteorological Station from the years 1989-1993 to make our calculations. These data were available in the appropriate format and therefore ready to use.

p. 8: Contrary to what is said in the first full paragraph, Litaor thought he found Pu in particle and colloidal form moving with groundwater in May/June 1995. He at least speculates, as I understand his work, that anoxic conditions of soil saturation may release some Pu into dissolved form. The second full para. on this page refers to this aspect of Litaor's work, but I wonder if it's correct to suggest that subsurface storm flow could be important only for "localized soil contamination areas," since seeps release material into stream channels that go to holding ponds or eventually exit the site. Also, it's not clear that channels have been adequately analyzed in terms of their ability to hold material flowing throught them; that is, do they leak?

We will review this section of the report to ensure that it is consistent with the literature.

p. 30: My note above about wind may be answered from RAC's perspective on pp. 29-30. But I raise a further question regarding RAC's assumption that "high winds will not be explored further in the SAL project." Why? Evidently because wind blows contamination away and thus lessens possibility of future resuspension by this means. OK. This makes sense, though it's not very reassuring news. But a decision to set aside further analysis re. wind seems predicated on the assumption that the 903 Pad will not release more and that main sources of resuspension have been already depleted. What about remediation of 903 area? What about taking down of buildings and exposure of whole new areas of contaminated soil? What about any construction activities that may occur? There seems to be ample reason to keep airborne resuspension alive as a very likely pathway for future exposure of unwitting populations. Am I missing something here?

We do not intend to eliminate the airborne resuspension pathway. The intent of this section of the report is to respond to the often heard comment about the severity of the high winds at Rocky Flats. It is true that wind speeds at Rocky Flats and in general along the Front Range in Colorado can reach very high speeds. What was learned in the dose reconstruction project, however, is that although high winds tend to resuspend a great deal of material, that material is generally dispersed rapidly. This rapid dispersion decreases the air concentrations at close to the source locations and thus decreases the dose to individuals that are of interest for this project. For that reason, we will not consider high winds, but rather average Rocky Flats winds resulting in resuspension.

p. 31: Re. scenarios, one peer reviewer in commenting on Task 2 raised a serious question re. "institutional controls." In a May 7, 1999, memo to RAC I raised the issue as follows: "One of the peer reviewers for the independent assessment of the Rocky Flats RSALs states that the

RSALs as adopted misapply the concept of 'institutional controls' in relation to the 15/85 mrem/year dose (see attached 'Review Comments on the March 1999 Draft Report . . . for Task 2: Computer Models," section 1, 'Application of the 85 mrem/y criterion'). This suggests that the Rocky Flats RSALs violate CERCLA in the way the 'institutional controls' concept is employed. What corrections need to be made?" I raise this question anew because it was not previously answered and because it comes up again under "scenarios." One of the scenarios included in the officially adopted RSALs – the hypothetical future resident -- assumes disappearance of institutional controls, in possible violation of CERCLA, if the peer reviewer is correct in the comment submitted. If the reviewer is correct, then the hypothetical future resident scenario (as well as all other hypothetical future scenarios) needs to be recast in terms not of a possible dose of 85 mrem/yr but of 15. How does RAC respond?

This same reviewer brought up this topic again. We respond by reminding the panel that we will present distributions of soil action level for both dose criteria for all scenarios. The panel and *RAC* can then work together to develop recommendations to DOE.

pp. 34-36: This section does not make sense to me. Table 11 shows breathing rates ranging from 7.5 L/min to 712. Is this correct? The numbers given on p. 36 seem far less than those provided by Joe Goldfield January 31, 1999, paper. Joe's paper has the virtue of clarity and persuasiveness. I defer to him in the hopes he will make a clear response to this section.

Thanks to the reviewer for noting this typographical error; a hyphen was missing and it should read 7–12. The appropriate change will be made in the final report. The breathing rate distributions shown in Figure 6 in the report were those the panel agreed upon at the May 1999 meeting, following several months of intense panel discussion and the consultation with a specialist in respiratory physiology at CSU.

pp. 37-40: Re. soil ingestion, I again defer to Joe Goldfield.

As with the breathing rate distributions, the distribution of soil ingestion rates and the selection of the value for use in the scenarios was approved by the panel at the May 1999 meeting. We considered many published reports, along with Joe Goldfield's paper he wrote for the panel, in our assessment.

DOE COMMENTS

Comments and Questions on *RAC*'s Draft Report for *Task 3: Inputs and Assumptions*

1. Pages 4 through 10 of the draft Task 3 report summarizes the results of a sensitivity analysis, but does not provide the full documentation that lies behind this analysis. At the RAC Sensitivity Analysis for RESRAD Parameter presentation on January 14, 1999, the most sensitive parameters were identified as solubility of plutonium/dose conversion factor and the mass loading factor. The less sensitive parameters were identified as cover depth, breathing rate and soil ingestion. During the Project Update presentation in May 1999, the impacts between using RESRAD v5.61 and 5.82 were identified. The documentation supporting the sensitivity analysis is needed to understand how RAC classified the parameters as discussed on page 4 of the Task 3 Report without having an independent reviewer repeating each sensitivity analysis. Please provide in the final report documentation supporting the sensitivity analysis.

We will include a more detailed discussion of the sensitivity analysis in the final version of the Task 3 report.

2. RAC has recommended an "Indoor Dust Filtration" factor of 1.0 (page 5). The Rocky Flats Cleanup Agreement (RFCA) Parties have identified new information from both EPA (Exposure Factors Handbook) and NCRP (NCRP Report No. 129) that may impact this input and are evaluating this information as part of the RFCA annual review process. Has RAC evaluated the new information available from the EPA and NCRP as it relates to this parameter?

We are exploring a distribution of values for the final version of this report as a result of the significant number of comments on this parameter. We thank this reviewer for identifying additional documentation to assist us in this task.

3. Table 2, "Relative Concentration of Radionuclides in Soil at Rocky Flats in 1999," could not be verified with the information and references provided in the draft report. Please include in the final report the data representing how the mass values from the references listed were converted to activities and allowed to decay (or grow in, in the case of ^{241}Am) to the year 1999 for use in the RESRAD calculations.

Because this reviewer could not reproduce the values in Table 2, we will review the calculations to ensure that they were done correctly. The conversions from mass to activity were done using the latest available specific activity values, decay occurred via radioactive decay (using the latest available half-life values) and including a generic weathering constant of 4.0×10^{-4}.

4. It is not clear from the Task 3 report how RAC plans to analyze the agency scenarios. Specifically, it is not clear if RAC plans to substitute its own parameter values for the agency values (as shown in Table 4) in calculating new recommended RSALs for the agency scenarios. Can RAC clarify this issue? Also, Table 10 lists the different Scenario Parameter

Values for DOE and RAC scenarios. It is not clear from the table or from the text if RAC concurs with or is simply not analyzing the parameter values for the DOE scenarios. For example, the agencies assumed for an Open Space scenario a value for time on site of 125 hours per year. By not adjusting this parameter, is RAC endorsing it or simply choosing not to analyze it? Or has RAC concluded that it is not sensitive and therefore does not merit more detailed analysis? In other words, does RAC agree that the agencies have appropriately defined their own scenarios, or for the purpose of analysis is RAC simply accepting the Scenario parameter values as is?

We plan to analyze the agency scenarios and the *RAC* scenarios using the scenario parameters presented in Table 10 and the site-related parameters presented in Table 4 (*RAC* value column for all scenarios) and the accompanying text. The agency scenarios were in close agreement with similar *RAC* scenarios that were previously developed but subsequently dropped because of their close resemblance to the agency scenarios. The determination of the scenarios by which to evaluate soil cleanup levels is to be made by the panel after presentation of results of the analysis for all scenarios.

5. The Actinide Migration Team has recently completed work directly related to Kd values. We attached a copy of the report that we believe is relevant to the Task 3 report.

Upon receipt of these comments, we requested and have received a copy of this report. We thank this reviewer for bringing this report to our attention and plan to evaluate it and possibly incorporate the results for the final version of this report.

6. RAC has defined a model of ^{239}Pu concentration in soil as a function of location (page 20). Do similar models need to be defined for ^{241}Am or U? If yes, what task report will explain this extrapolation? If not, will the Pu data be extrapolated for Am and/or U?

Americium and uranium concentrations will be extrapolated from this model based on the radionuclide ratios given in Table 2.

7. Figure 2 represents the locations of more than 588 soil samples of ^{239}Pu at Rocky Flats which were used as a basis for a spatial model. While the text states the sources of the raw soil concentration data, the text also states that the 588 soil samples are a subset of the raw soil concentration data (page 22). Please provide in the final report a list, including the source, of the 588 entries.

The database of the soil samples used to create this distribution was defined for the Phase I dose reconstruction project, and is outlined in the ChemRisk Task 6 report (1994). Additionally data was needed to supplement this historical database, and those data were obtained from the data set deposited by M.I. Litaor with the Colorado Department of Public Health and Environment. These supplemental data were used to enhance the resolution of measurements available at locations near the 903 Area.

8. RAC's recommended breathing rates (page 36) could not be verified with the information in this report. As captured in the RAC Scenario presentation on January 14, 1999, it is

important to understand the duration of daily activities for each receptor in order to calculate a breathing rate. For clarity, please incorporate the assigned duration for the various daily activity levels in the final report. Also, please incorporate the distributions of breathing rates for active and sedentary adults, for active and sedentary children, and for active and sedentary infants (as captured in the RAC Breathing Rate Distributions presentation on March 11, 1999) in the final report. Please also explain why and on what basis RAC recommended using the 95th percentile value from the breathing rate distribution.

The selection of breathing rate values for the scenarios was a long process involving many discussions with the panel and consultation with a respiratory physiologist. In developing our breathing rate distribution we reviewed numerous reports as described in the Task 3 report. We did develop detailed breakdown of time/activity levels for each scenario and have that information available. We will consider the reviewer's request to include those detailed spreadsheets in the report.

9. RAC recommended identical annual soil ingestion values for each of RAC's recommended scenarios, i.e., current site industrial worker, resident rancher, infant of rancher, and child of rancher (page 39). Is it possible to create a frequency distribution of soil ingestion values for each scenario similar to what was done for breathing rates?

We did create a distribution of soil ingestion across the population, but based on the types of information available on soil ingestion, it was not reasonable to create the same type of frequency distribution based on scenarios.

10. The RAC recommended consumption rates for fruits, nonleafy vegetables and grains (page 40) could not be verified from NCRP Report 129. Please state where in NCRP Report 129 these ingestion rates were taken. There is currently no reference for the RAC recommended leafy vegetable consumption rate.

We will make the appropriate revisions in the report so that the source of these values is clearly referenced.

11. RAC states on page 27 of the draft Task 3 report that monitoring data do not provide particle size information. Since 1995, the Kaiser-Hill Team has been reporting, in the Quarterly Environmental Monitoring Report, air monitoring data from selected locations and time periods at the Site that contain size-segregated radionuclide concentrations, separated at about 9 to 10 micrometers. Has RAC evaluated this information as it relates to this parameter?

This information was not available to use at the time the production of this report was completed. We would like to receive this information, but it is not clear that we would be able to use it in the final modeling effort for this project, which is already well underway.